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PERSONAL

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Dear John:

Thank you for your letter of June 30th and for bringing me into your dialogue about the NIH peer review problems.

I certainly share your sense of concern about this problem: Not only is it one of grave practical import but it also strikes directly at the integrity of the internal governance of science. In fact, I had intended to devote most of my talk in November to similar issues. I am very well aware of the admonitions summarized on page 8 and they have to some extent inhibited me from speaking out as critically as I feel on some of these questions. In fact my personal feelings are sufficiently exercised that I must take the precaution of asking you to regard these remarks as private until I have had considerably more time to develop my settled opinions and statements.

It is probably superfluous for me to recapitulate the context of civic culture and government policy in which the peer review system must operate. The general decay of civility and the explosion of litigiousness remind one of Aristophanes' "Wasps". The problems are aggravated by structural anomalies: Imbalances between training and funding, rigidities in academic career structures and perhaps especially by desperation about the continuity of programs. To talk about the "deferral" of a grant is rather like holding one's breath. Under the federal grant system, institutions, unless they have remarkable internal resources, have no buffering capability. Unlike the private sector, they cannot gamble on investments based on their own self-confidence with the possibility of getting future profit and retroactive reimbursement even if their confidence was justified. Nor is there any mechanism to allow for savings from the profits on current operations to form a reserve for future fluctuations. This basic structural problem, coupled with the displacement of private philanthropy by government support itself, suggests that we may need some major new institutions to cope with the on-going support of the scientific establishment. Plainly, government must be held to the strictest rules of accountability, public access, and annual budgeting. I can see no political or constitutional reason why there could not be a substantial delegation of responsibility to a government funded not-for-profit corporation to act as an intermediary

in coping with some of the fundamental conflicts between the way that government must operate and sensible procedures for academic research.

Although I have been a frequent victim of the "system", as well as a grateful beneficiary, I would have to admit that it is hard to prove that the system is unfair or that the net outcome of allocation of funds is inefficient. What there can be no doubt about is the enormous consumption of energy, time, and anxiety which leaves so many of our colleagues in a state of permanent application-pending psychology. The paper refers to an administrative cost of 1% of expenditures at the NIH. This is matched with what I would judge to be at least 10% per grant year after year on the part of investigators and this is perhaps an under estimate. (Privately, I could remark that I confidently expect to be spending no more time in fund seeking as the President of a university than I was doing habitually as the leader of a laboratory and principal investigator of a few research and training programs in the department of genetics.) This is not only an objective waste of energy; it also inevitably entails a serious influence on the personality and character of the effective practitioners of science .

There are some general policy directions within NIH that might be feasible and might offer some improvements. The director might seek to return as a matter of explicit policy to a former tradition of supporting tested investigators for further creative work, of a kind that is inherently unpredictable if it is to be of real scientific substance. So many proposals that have any real depth are inherently a charade if they purport to be able to describe the experiment that is appropriate to do the day after tomorrow! I cannot recall a single instance in my own career where it was desirable or possible to do the precise experiments that had been outlined in a proposal, simply because of the new facts and insights that fortunately arrived during the interval between the writing and the initiation of the project. But I have been very badly burned when I honestly attempted to portray this situation, and have had proposals returned to me as "insulting" when they lacked the explicit details of fictitious experiments.

There are frequent references to the expression "peers", in the text; but I wonder if they take realistic account of the actual composition of Study Sections today. This maybe a paranoid suspicion on my part; and it deserves to be checked out carefully and objectively. But the very trends quoted in the text, that discourage the continued participation of creative investigators on the peer review panels, must inevitably attract another cadre of critics whose own creative capability was blunted either by inherent factors or by external frustrations that would exacerbate their resentment. At a time of declining funds this also tends to deepen generational (and other group) conflicts to the point where current trends, if continued, may result in review bodies that are no longer "peers"

manifest
of established scientists, but rather angry and resentful critics. I would, of course, concede that this characterization can, at the present time, attach only to a small minority but it is a concern that should not be swept under the rug. In its mildest form, the generational conflict is, (I am certain) in the assumption that "seniors" will be able to get by, from magical resources; and that a review body need not look too deeply into the consequences of terminating grant programs that had been renewed for a number of years previously. This is likely enough that I know that most of my older colleagues pursue the avowed strategy of farming out a variety of grant applications to their younger colleagues rather than dare to accept centralized responsibility themselves. This of course only exaggerates the number of applications that need to be reviewed, multiplying the work load and further splintering the allocation process.

From the platform of my own experience on the NAMHC some years ago, I have long been convinced that the councils could and occasionally do play a key role in ameliorating these problems. They could and should be effective appellate groups; and if there were some confidence that malice or misunderstanding could be ameliorated in another place there might well be less anxiety about interacting with the system in a disturbing way at earlier stages. On the other hand, I firmly believe that any system that gives absolute power to any group, like an IRG, must, according to well known doctrine, become corrupt. For that reason I would strongly resist the closure on feedback that is recommended at many places in the text. In order to be able to play an effective role the councils must be carefully composed; even more they must have effective staff and technical support. There is nothing clumsier than the present system for handling the vast quantities of information that are the main stream of the review process. I simply cannot understand why microforms are not used routinely as one measure of economy and portability in the exchange of documents so that they could in fact be readily available in a timely way to whichever IRG reviewers and council members could take an interest in them. The councils must meet much more often for there to be any reasonable flexibility in dialectic between the investigator, the IRG, and the council itself. At the present time, a deferral is a kiss of death given the very long intervals that now pertain to any significant exchange of information and decision. There are plenty of technical aids to efficient communication which any sensible business would have adopted long ago if they had to cope with similar problems. But I am told that the fiscal pressures on the administrative side, and the particularly artificial one that capital investments must all be budgeted in the year of adoption, have frustrated any serious reexamination of the management technology. ^P I would strongly endorse the need to sustain the morale of the NIH staff but I believe that they are simply unable to take care of the tasks assigned to them without far better methods than they are now able to use. In fact these methods are getting worse if one considers the serious deterioration of the postal service and the moderate decay in telephone audibility (albeit coupled with reduction in real cost) that we have experienced in the last decade.

I would be glad to be engaged in further critical analysis of the situation: It is encouraging that there is recognized now to be a serious problem and that we can then go into a problem-solving mode in trying to

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meet it. I would suggest that, besides affected scientists and administrators, we recruit the talents of some expertise in information management as well as in the social psychology of peer review. (For the latter, I have in mind people like Harriet Zuckerman and Jonathan Cole).

I am also enclosing a few tidbits of other writings that reflect on the present situation and as well give some substantiation that my views are not the immediate product of my new administrative responsibilities.

Attached also is a list of specific comments keyed to paragraphs in the text that you sent me.

This has been a provocative and instructive exercise for me; and as I will be continuing to prepare my remarks for the November meeting I would be most interested in any other material that you would care to send.

Your letter is also a partial answer to a query that I sent either to you or to John Cooper a while ago, asking about the role of AAMC in relation to biomedical research institutes in contrast to medical schools. But I still do not have a clear picture of any formal arrangements for institutional affiliations along those lines.

Yours sincerely,



Joshua Lederberg

JL/gel

enclosures